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The Social Psychology of "Pseudoscience": A Brief History

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INTRODUCTION

This paper is about the social and cultural settings of the word "pseudoscience". Like "paedophile" and "terrorist" it has an etymologically transparent sense, and during the twentieth century, it was used rhetorically to refer to an activity falsely claiming scientific status. Thus: "The government is using a pseudo-scientific justification of GM to conceal its acquiescence to global, corporate control of key food supplies" (Letter to *The Guardian*, Thursday, Mar 4, 2004). But from time to time such words have occurred in a more formal, technical sense around a perceived threat to individual and institutional security. It is these foci of activity in the use of "pseudoscience" that we are interested in.

Twenty years ago the philosopher Larry Laudan (1983) announced "The demise of the demarcation problem". This problem was to demarcate between science and pseudoscience, and Laudan showed that it is impossible to arrive at a definition of science which will distinguish all scientific from all pseudoscientific or nonscientific statements. There is no scientific essence whose presence or absence can distinguish the two. If he and others[1] who shared his view were right, then a decline in serious discussions of the problem would be expected. But the reverse has recently occurred in applied areas of human and biological sciences, such as psychology, psychotherapy, and medicine. However insoluble by philosophical standards, demarcation remains troublesome, generating passion amongst those who speak on the side of science, and feel the need to separate it clearly from other activities. There are currently several attempts posted on the internet to list diagnostic criteria for detecting pseudoscience, as though it were a kind of pathology.[2] Recently the label "pseudoscience" has been used to damn new therapies, such as antiageing therapies (de Grey et al., 2002), facilitated communication (Jacobson et al., 1995), Eye-movement Desensitization and Reprocessing (EMDR; Herbert et al., 2000), and even the well-established Rational-Emotive Behavior Therapy (Wessler, 1996).

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EMDR is an apparently evidence-based therapy for Post Traumatic Stress Disorder; in 15 years it has become popular in the States and elsewhere, and now claims successful treatment for a wide range of psychological problems. The paper by Herbert et al. stands out as heavyweight by any standards, unremitting in its denunciation of the poor evidence, the crude theory, and the dubious use of publicity. The attempts by the supporters of EMDR to explain away negative results cannot merely be dismissed as bad science, to be remedied by the self-correcting processes inherent in science. They are symptoms of pseudoscience, which entails exile from the domain of science. To prove their point, they appealed to the same criteria that had earlier been found wanting by Laudan and other philosophers.

Two of the authors of the paper on EMDR have coedited a book designed to fight the erosion of the "scientific foundations of clinical psychology" (Lilienfeld et al., 2003, p. 1), and a whole issue of *Scientific Review of Mental Health Practice* (2004) has recently been devoted to the danger of pseudoscientific tendencies. The urgent, combative tone of some of these publications is unmistakable, and a contrast to the detachment of philosophers and sociologists who dominated the discussion around 1980.[3] What has changed? To answer this we will turn to some earlier history of boundaries set up to protect science.

The psychological theory behind our analysis is that derogatory labelling of others often includes an unstated self-definition. Thus a negative judgement that EMDR is a pseudoscience can serve to fortify the speaker's sense of self as scientist, sharing such judgements with similar scientists. This is the basis of projection and reaction formation, though we derive it from the theory of mutualism (Still and Good, 1992), in which "The awareness of the world and of one's complementary relations to the world are not separable." (Gibson, 1979, p. 141). A comforting identification with science may or not be valid. For the purposes of this essay it is valid if there really is a distinct class of scientists to which the writer belongs, and the activities of members of this class have a unique value. It can therefore be valid if science is a unity, and has special access to knowledge not shared by pseudoscientists. If it is not valid according to these criteria, use of the word may still bring comfort, derived from the informal sense referred to earlier.

A sociological version of this comfort-seeking, which avoids the question of validity, is that

> . . . the allegation of "pseudo-science" is . . . an aspect of the rhetorical and organizational means by which scientists and other sympathetic agents seek to distinguish the domain of science, and to discredit some other activity, in the process of maintaining and extending the status, cognitive authority, funding, autonomy and other interests and values of those associated with science as an enterprise (Wallis, 1985, p. 593).

PREHISTORY OF PSEUDOSCIENCE

The word "pseudoscience" first became widespread in the 20th century, but it occurred earlier,[4] and reflects a dynamic that is older. This is the attempt to define

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science as something distinct from other activities, with a unique importance for generating knowledge. This has been described as a mapping process (Gieryn, 1999), drawing boundaries to keep science apart from non-science. But it is more than just mapping, since the high status of science means that space allotted to it is not neutral; by granting "epistemic authority" (Gieryn, 1999, p. x), it conveys privilege for any activity established there. Inclusion or exclusion can determine the future of a discipline, and even its survival.

Darnton (1984, p. 189)[5] has described how Diderot and D'Alembert, the philosophers mainly responsible for the Eighteenth century *Encylopédie*, used two metaphors, the tree of knowledge with its different branches, and a map with boundaries and interconnections. We will be exploring the tension between these two metaphors in writings about science. On the map metaphor, activities excluded from the area reserved from true empirical knowledge are allocated a different space which may in principle be just as fertile. On the tree metaphor, there is not only a hierarchy within the tree, but discarded branches are left to wither, expelled from the organic whole that could give them real life in a world that lives on knowledge. Or, less fancifully, they are excluded from the fertile soil of life-giving research grants. Compared to earlier classifications,the *Encylopédie* gave science a new and prominent place at the expense of theology, a process which Darnton called, in the title of his chapter, "Philosophers trim the tree of knowledge" (Darnton, 1984). Theology was no longer an essential part of the organic whole, and this paved the way for its severance altogether from the main tree.

THE NINETEENTH CENTURY

During the nineteenth century there were attempts to uncover what is essential in scientific activity that separates it from nonscientific activities. John Stuart Mill (1898; first edition, 1843) laid down canons of experimental reasoning, Whewell (1840) proposed "consilience" to describe scientific thinking, while the more practical John Tyndall publicly campaigned to enhance the prestige of science above mechanics and religion (Gieryn, 1999).

Defending science against popular suspicion, Herbert Spencer reassured his public that ". . . it is simply a higher development of common knowledge" (Spencer, 1904, p. 13; first written in 1860, according to the preface), even if something that seemed different from this had emerged. Readers of learned publications did not have to look far to see this in concrete detail, and to form their own view of how different from common knowledge it could be in practice. A striking example was Robert Koch's work which established the germ theory of disease. As well as an important theorist, Koch excelled as a technician, inventing techniques basic to modern bacteriology. Using these new techniques he described the life cycle of the anthrax bacillus, discovered the tubercle bacillus, and isolated the bacterium

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which causes cholera (Brock, 1988, p. 3). His discoveries became accepted through the development of styles of argument for establishing causal connections between germs and disease:

> To prove that tuberculosis is caused by the invasion of bacilli, and that it is a parasitic disease primarily caused by the growth and multiplication of bacilli, it is necessary to isolate the bacilli from the body, to grow them in pure culture until they are freed from every disease product of the animal organism, and, by introducing isolated bacilli into animals, to reproduce the same morbid condition that is known to follow from inoculation with spontaneously developed tuberculous material. (Koch, 1987, p. 87; first published in 1882).

This excerpt from a lecture formed the basis of what are now well known as Koch's postulates, and on finishing ". . . the audience was left spellbound, and for a time after he had ended the presentation not a word was uttered" (Lechevalier and Solotorovsky, 1974; quoted in Koch, 1987, p. xx).

But he was soon obliged to defend himself against failures to confirm his results. He dismissed these with scornful sarcasm, mostly against "the remarkable . . . reception of the new theory by American researchers" chiefly "the great microscopist Schmidt" (Koch, 1987, p. 118), and concluded that there had been no research

> . . . that disproves, even in the slightest, my claims about the etiology of tuberculosis. It was not pleasant for me to criticize such an entirely worthless body of literature (Koch, 1987, p. 126).

Harsh words for a man who had been closeted for so many years with those paradigms of unpleasantness, the tubercle and anthrax bacilli.

While the scientist Robert Koch was helping to establish the germ theory of disease, William Radam was constructing a different story from his own version of the theory (Young, 1961). Having emigrated from Prussia to Texas he worked as a nursery gardener, developing his 30 acres in an enterprising, experimental fashion. When he became ill with malaria and sciatica, he took the same approach to this problem that had brought success in horticulture. Microbes that attack the body, he reasoned, are like bugs that attack plants; therefore

> . . . if I could discover any thing that would kill blight, fungi, and microbes on plants without injuring them, I should also be in possession of something that would cure me (Radam, quoted in Young, 1961, pp. 145–6).

With a passion fired by the prospect of the loss of his livelihood and early death, he tried many substances before discovering his "Microbe Killer". This appears to have been a mild antiseptic which

> . . . could be taken in such huge amounts that it would saturate all the tissues and permeate all the blood of the human frame, and this with safety to the person, with destruction to the microbes . . . (Young, 1961, p. 146).

Radam and his microbe killer gained a local reputation, on which he capitalised by patenting the process and packaging it for a much broader market.

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Radam was in a long tradition of patent medicine maufacturers who boast scientific credentials. Fortunes were made, but success attracted the attention of investigative journalists and reforming politicians. There had been laws for some time against false statements about the ingredients of a proprietary medicine, but no obligation to list its composition, even if, as often happened, they included alcohol or opium (Young, 1961, p. 227). Energetic campaigns led to the Pure Food and Drugs Act of 1906. This tightened the law on misinformation, and required manufacturers to declare on the label the presence of dangerous drugs. Regulation has increasingly required the application of scientific method. Chemistry and biological sciences are used in the production and regulation of medicines and food, and experiments drawing on modern statistical techniques are required to support therapeutic and nutritional claims. Nowadays those treated as experts on these matters are popularly referred to as "scientists".

Koch and Radam will serve as two type specimens on which to base our discussion of science and pseudoscience; it is a change from the more usual appeal to physics as the embodiment of science, and more appropriate for understanding the recent alarm about pseudoscientific tendencies in new therapies. Both drew on the current germ theory of disease, but Koch was recognised as an exemplary (though sometimes unlovable) scientist, while Radam was an amiable entrepreneur who used aspects of scientific language and practice to convince himself and his public of the value of his product.

THE RISE OF PSEUDOSCIENCE

Big and little pseudoscience

Writings on pseudoscience[6] during the twentieth century fell into two overlapping traditions, which we will refer to as big and little pseudoscience.[7] In little pseudoscience the concept was applied to activities centred around claims and ideas that are commonly seen as bizarre and cranky, at least in retrospect, but have a large following for a short period of time. They laughably fail as science, so demarcation is not a serious issue, and calling them "pseudosciences" is usually uncontroversial. Like weeds they are easily identified, but can threaten cultivated plants if the gardener is not vigilant.

In discussions of big pseudoscience the concept has been linked to the philosophical issue of demarcation, of how science is to be distinguished from other activities. The other activities in question tend to be intellectually challenging and to attract an educated audience. The classic candidate was Psychoanalysis, though others have included Marxism, Psychometrics and Biological Psychiatry. Historically Astrology and Phrenology have been big pseudoscience, though their modern residues are little pseudosciences.

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Tales of little pseudoscience

The modern classic in this genre is Martin Gardner's *Facts and Fallacies in the Name of Science*.[8] There was already a tradition of exposing scientific pretensions, but Gardner's was the prototype of a new popular genre. He described 24 examples, each contained within about 15 pages; they included Flying Saucers, Lysenkoism, Orgonomy, Dianetics, General Semantics, ESP, Food Faddists, and Medical Quacks. His motive was educational, to immunize the public against the infection of pseudo-science, by helping us to recognise the future carrier when first encountered. There is a problem but not yet dangerous, especially as "most pseudo-scientists have a number of characteristis in common (Gardner, 1952, p. 8); they "work in almost total isolation from their colleagues" (Gardner, 1952, p. 10) and have a tendency towards paranoia.

Young (1961) referred to quack medicine in the Nineteenth Century as pseudo-science, and Hines (1988) dealt with some of the same material as Gardner in more detail; he also included more big pseudoscience like psychoanalysis. He still put faith in education, though the stakes were higher:

> . . . the witch delusion and the Nazi horrors show the great damage done by uncritical acceptance of pseudoscientific claims. Both might well have been avoided if the public had been educated in critical scientific thinking. (Hines, 1988, p. 20)

Big pseudoscience and Unity of Science

Writings on science during the first half of the twentieth century were dominated by a modern version of the *Encylopédiste's* Tree of Knowledge; the Unity of Science movement, associated with the Vienna Circle during the 1920's and 1930's, and the unfinished *Encyclopedia of Unified Science*. There was no single philosophy behind this doctrine, and those associated with it such as Carnap and Neurath were more cautious than the bold title of their movement suggests (Creath, 1996). A folk belief in some form of unity is however reflected in the common use of the term "science" or "scientist", and the Unity of Science movement formalised this implicit belief.

The word "pseudoscience" emerged from the Vienna Circle and the logical positivism associated with it. In English the prefix "pseudo" was used to demarcate genuinely empirical statements from others. A pseudo-question was one to which there can be no clear answer, like "What is the meaning of life?" (Passmore, 1966, p. 373). The intention behind the prefix was not always perjorative. Wittgenstein (1922, p. 169) referred to the propositions of mathematics as "pseudo-propositions" without wishing to diminish them, and the literary theorist I.A. Richards (1974, first published in 1926) called poetic statements "pseudo-statements". This technical use of "pseudo" was the background to the use of "pseudoscience" in work by and about Karl Popper.

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Popper's formal education during the 1920's ranged from the history of music to philosophy, and his doctoral thesis was in the psychology of thinking. But throughout this time and earlier he was preoccupied with the nature of scientific knowledge, and he maintained close contacts with members of the Vienna Circle while he was developing the theories of *Logik der Forschung*, published in 1934.[9] "Pseudoscience" has been linked especially with his criterion of falsifiability for demarcating science from non-science.

Popper and demarcation; big pseudoscience as unfalsifiability

Popper's (1976) autobiography has provided the origin myth for big pseudoscience, in the story of his youthful search for a system of thought to contain his intellectual enthusiasms. First he was converted to communism during the unsettled period in Vienna immediately after the first world war. Its all embracing certainties promised a suitable framework, but he was disenchanted by the violence encouraged by the party. Later he was shocked at having allowed himself to become uncritically involved. He also failed to find what he was looking for in Psychoanalysis, which shared with Marxism the immodest claim to explain everything, and seemed able to turn apparent disconfirmations into their opposite.[10]

The climax and resolution of the story came in 1919 when the young Popper encountered one of the most spectacular events in the history of science, which exemplified for him the principle of falsifiability, that "*it must be possible for an empirical scientific system to be refuted by experience*" (Popper, 1968, p. 41, author's italics). This was the successful test of Einstein's eclipse predictions, which heralded a new theory, a real improvement on Newton (Popper, 1976, p. 37). The triumph was possible because Einstein's theory, giving a precise prediction in conflict with Newton's, had rendered itself so vulnerable to falsification. This was what the young pilgrim sought.

Popper described his principle of falsifiability as solving two problems. One was the ancient puzzle of induction. This arises if it is assumed that scientific knowledge depends on generalising from positive instances, e.g. generalising to "the suspect bacillus causes TB" from the observation that exposure to the bacillus was followed in several cases by the onset of the symptoms of TB; or to "the microbe killer cures many ailments" from the finding that after regular dosage of Radam's microbe killer the symptoms disappeared in well-attested instances. Popper's solution was that scientific knowledge does not depend on generalising from positive instances (which could put Koch and Radam on a par), but on a social practice in which theories and generalisations are challenged and put to the test by fellow scientists. They are trained to view theories critically, and to look for ways of falsifying it. We saw that process clearly in the case of Koch, whose colleagues in the United States and elsewhere seized on his claims and tried to discredit them, and its absence was equally clear in the case of Radam, the lone entrepreneur.

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For our story of "pseudoscience" the key move was to apply falsifiability to a new problem, which Popper insisted was the most important. This was to provide a criterion to demarcate science from pseudoscience. But the logical structure of falsifiability will not suffice as a demarcation criterion. Astrology, Marxism and Psychoanalysis make many statements which could in principle be falsified, but in practice (Popper believed) contrary results are assimilated by the theory concerned, and not allowed to act as falsifications. Radam's claims on behalf of his microbe killer were falsified, so must have been falsifiable, but this does not make his work scientific. No one doubted Koch's status as a scientist, yet he justifiably held on to his theory in the face of failures to confirm it.

Again, the social setting became crucial for Popper as he developed his views. He spelt this out in a fiercely ironic attack on the founders of the Sociology of Knowledge for ignoring the social aspects of science. His targets were Scheler and Mannheim, whose views (Popper believed) derived from the "Marxist doctrine that our opinions, including our moral and scientific opinions, are determined by class interest, and more generally by the social and historical situation of the time"; this is the "social habitat" which determines, in the form of an ideology, "opinions and theories which appear . . . unquestionably true or self-evident" (Popper, 1962, p. 213; first published in 1945). But they fail to see that their own views too must be equally determined by the social habitat,[11] and basing scientific knowledge upon the "consciousness" (Popper, 1962, p. 217; author's quotes) of the individual scientist, they overlook precisely the

> . . . *social aspect of scientific method*, with the fact that science and scientific objectivity do not (and cannot) result from the attempts of an individual scientist to be "objective", but from the co-operation of many scientists. (Popper, 1962, p. 217; author's italics).

Scientific objectivity, like liberal democracy, is constituted by free criticism and social institutions.

> Only political power, when it is used to suppress free criticism, or when it fails to protect it, can impair the functioning of these institutions, on which all progress, scientific, technological, and political, ultimately depends (Popper, 1962, p. 218).

This account of science as falsifiability, demarcating it from pseudoscience, has had lasting appeal to scientists (both Herbert et al., 2000, and Wessler, 1996 appeal to it), but difficulties still remained when the social aspects were made clear. In practice theories tend to be held on to rather than jetisoned when disconfirmed, and even Popper's mythic paradigm of falsifiability, the testing of Einstein's eclipse predictions, may have been distorted by a "confirmation bias" (Shermer, 2002, p. 301, quoting Collins and Pinch, 1993). Popper did recognise that there is a conservatism in science, and that theories can be clung to with great tenacity in the face of apparently falsifying evidence. He made a virtue of it within his theory; conjectures need to be given a chance to strengthen themselves before

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they are finally refuted in the face of a testing process which will itself gain strength from encountering determined theories. But the question remained, when is resistance to falsification in the face of disconfirmation consistent with good scientific practice, and when is it the mark of a pseudoscience?

The need to resolve this difficulty was made urgent by the impact of Thomas Kuhn (1962), since he gave the impression to many readers that Popper's principle of falsifiability had little to do with scientific progress. According to Kuhn science does not advance through a steady process of "Conjectures and Refutations". Instead normal science is always framed within a set of "paradigms" of theory and practice, and busies itself with solving the problems thrown up by the paradigms. Anomalies are ignored, and theories whose rejection would threaten the paradigm will be held on to even in the face of apparent falsification. But sometimes anomalies become too glaring, and this crisis may be resolved through a revolution, giving rise to a new paradigms, incommensurable with the old. This historical theory seemed to put a distance between nature and scientific practice, and to undermine Popper's principles of demarcation. The apparent threat to science's reputation triggered energetic attempts to reconcile Kuhn and Popper, and to re-establish boundaries between science and pseudoscience.

Big Pseudoscience as degenerating research programme

The best known attempt was by Imre Lakatos (1970) in his theory of scientific research programmes. The hard core of a science is preserved by a protective belt, created by the negative heuristic which points research away from the hard core, and the positive heuristic which directs progressive work towards new theories and discoveries. In this way a scientific theory is protected to some extent against falsification; its auxiliary hypotheses which mediate between theory and experiment can be adjusted to allow for discrepancies. But there are limits on the adjustments that can be made. A research programme is progressive if its theories predict new facts, and lead to the actual discovery of new facts. Otherwise it is degenerating, and "pseudoscientific" (Lakatos, 1970, p. 118). A serious problem arises when it indulges in "pseudoscientific adjustments" with no restraint (Lakatos, 1970, p. 117). The examples taken by Lakatos were from Physics, and suggest that his normative vision of progressive and degenerating research programmes was specific to that discipline. But social sciences can take little comfort from Lakatos' delineation of pseudoscience. In the conclusion to his long essay, Lakatos dismissed Marxism and Freudism as pseudosciences, and also "modern social psychology", since they are not progressive. "They do not add up to a genuine research programme and are, on the whole, worthless", and he

> . . . wonders whether the function of statistical techniques in the social sciences is not primarily to provide a machinery for producing phoney collaboration and thereby a semblance of

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> "scientific progress" where, in fact, there is nothing but an increase in pseudo-intellectual garbage (Lakatos, 1970, p. 176).

He pursued the garbage metaphor to a Draconian conclusion:

> . . . the methodology of research programmes might help us in devising laws for stemming this intellectual pollution which may destroy our cultural environment even earlier than industrial and traffic pollution destroys our physical environment (Lakatos, 1970, p. 176)

Mario Bunge (1967), a physicist as well as a philosopher of science and of mind, was equally severe on pseudosciences, though did not (at this time) go as far as Lakatos by suggesting legislation to control them. He showed little interest in the niceties of the demarcation problem, but took the existence of pseudoscience for granted, and asked "What is wrong with pseudoscience?" He answered that it refuses to ground its doctines in science and to test them by experiment proper; it lacks a self-correction mechanism, and its aim is to influence people rather than to map reality—criteria general enough to include both psychoanalysis and dowsing.

Big Pseudoscience as ideology

Sometimes a scientist may be tempted to maintain a position in the interest of some external power, like the organisation that provides financial support, or the beliefs of a political dictator. A much-cited example of the latter is Lysenkoism, which was supported by Stalin and became the dominant biological theory in the USSR during the 1930's. Such external control is often called "ideological".[12]

Blum (1978) used "ideological" in this sense, and considered mental testing (which he called "eugenics/psychometrics" to bring out its supposed racist origins) to be a pseudoscience for this reason. Like Lysenkoism, eugenics/psychometrics has been officially sanctioned; its "domain is an entire nation or group of nations, and the false beliefs are openly professed as valid science by respected authorities in major educational institutions." (Blum, 1978, p. 146). In a review of Blum (1978), Cooter (1980) argued from a neomarxist viewpoint that the deployment of "pseudoscience" always acts conservatively because "it legitimates the world-view that is mystified and mediated through science" (Cooter, 1980, p. 259). But although this may be the primary force of the word, it can also be used in a secondary sense, to make a rhetorical distinction even without any committment to science. Thus the power of psychometrics depends on its scientific pretensions; therefore a successful attack on it as "pseudoscience" would undermine it (and its racist influence) in the eyes of those who accept the worldview that is mediated through science. So it acts conservatvely through such eyes, but Blum was making use of this action in the interests of an anti-racist argument. The self-definition of Blum (a sociologist) was of himself as anti-racist, rather than as scientist.

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Ross and Pam (1995) argued that biological psychiatry is a pseudoscience because it ". . . is dominated by a reductionist ideology that distorts and represents much of its research" (Ross and Pam 1995, pp. 1–2). The assumed political origin of the inherent bias was spelt out clearly:

> Human choices and values are negated, and the sociocultural status quo remains intact. Thus, an overly biologized psychiatry becomes an instrument with which to repress alternative psychosocial models of psychopathology (Ross and Pam, 1995, p. 3).

They did not pretend to be neutral. Their book was written from within, to promote a psychosocial model by subverting the dominant views stifling it.

Big pseudoscience as the art of the insoluble

Writing as psychologists, Leahey and Leahey (1983) saw pseudosciences as "psychological movements that have been rejected by established psychology" yet continue to attract many followers and "form a dark, unconscious side to establishment psychology" (Leahey and Leahey, 1983, p. 14). There are no formal demarcation criteria, since boundaries are established by social consensus rather than by the possession of any special access to truth possessed by science. Appealing to Kuhn, they argued that normal science follows its paradigms, and therefore ". . . *is* a collection of subjects approved for research" (Leahey and Leahey, 1983, p. 238; authors' italics) and ". . . content rather than form or method determines which fields are considered to be pseudosciences" (Leahey and Leahey, 1983, p. 239). Science is not a search for ultimate values, and pseudoscience arises from disatisfaction with this, and a "deeply rooted human need for a universe of meaning, rather than one of mere order" (Leahey and Leahey, 1983, p. 245). Pseudosciences like phrenology, mesmerism, and parapsychology ". . . are not sciences because they try to delve deeper than any science can" (Leahey and Leahey, 1983, p. 245). Although they did not refer to Medawar, they might agree with him that science is the art of the soluble (Medawar, 1969).

Summary

These examples of big pseudoscience show how the uses of the word reflect the critical and scientific interests of the writer. For philosophers as identified with physics as Popper, Lakatos, and Bunge pseudoscience was no real threat, but only as long as there are secure boundaries; the demarcation problem was therefore of abiding importance (though not problematic in the case of Bunge).

By laying down rules, Lakatos hoped to supplement and enhance the self-correcting processes of science, and to ensure that lapses could be more easily detected. Blum and Pam et al. targeted a less obvious danger, where self-correction

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may look healthy, but where external forces are being used to "suppress free criticism" (Popper, 1962, p. 218). Blum's stance was from outside science, and he used "pseudoscience" to undermine the scientific pretensions of an established sub-discipline, and thereby strengthen the anti-racist position in the IQ debate of the time. Pam et al's was a critique by practitioners, using the label in an attempt to revive and secure their own beleagured position as psychosocial theorists. Leahey and Leahey described a less sinister form of "pseudoscience", where the failure lies in selecting the wrong kind of problem, of seeking to uncover secrets that lie beyond the art of the soluble. Their labelling served a cleansing process, acknowledging ownership of psychology's shadow side in order to keep it safely in its place, leaving psychology (and themselves as psychologists) secure.

BREACHING THE BOUNDARIES

The Sociology and Social Psychology of Scientific Knowledge

The Leaheys followed Kuhn rather than Popper or Lakatos, and were in tune with some of the dominant views around 1980. Essentialism was out of fashion, and philosophers had become lukewarm about the demarcation problem. Sociology of Scientific Knowlege (SSK) was now well underway; if it had been crushed by Popper's attack on Scheler and Mannheim, it had certainly recovered after being established as an empirical and historical discipline by Robert Merton. It had moved on, from Merton as well as Mannheim, in several respects. It tried to study empirically the causes of scientific knowledge and belief; it was self-consciously reflexive, recognising that the same questions about knowledge and belief can be asked of itself; and finally, it assumed "symmetry" between the causes of true and false beliefs, between science and pseudoscience (Bloor, 1976). As Bruno Latour put it, in his third rule of method for studying scientific controversies and their outcomes: "since the settlement of a controversy is *the cause* of Nature's representation not the consequence, we *can never use the outcome—Nature—to explain how and why a controversy has been settled*" (Latour, 1987, p. 99; author's italics).

Rom Harré (1986) drew on the empirical work of SSK to develop his account of science as a communal practice. This approach to science might seem akin to Popper's appeal to intersubjectivity, but for Popper the communal activities were a realisation of the abstract principle contained in falsifiability and "conjectures and refutations". Harré, on the other hand, followed Polanyi (1962) in his characterisation of a scientific community, and in his "fiduciary transposition": "Assertions of fact. . . . are to be read not as 'This is true' but as 'Trust me when I say that'" (Harré, 1986, p. 19). If it is reasonable to trust scientists (usually), it is not because they follow strict, rational principles. On the contrary, he noted from

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the SSK studies how often the personal qualities of scientists are appealed to in judging the reliability of results, and how there is asymmetry in the use of criteria for the support of one's own ideas, and those of a rival (Harré, 1986, p. 85). This certainly shows how work can come to be excluded as unscientific, but not by the strict principled criteria of demarcation sought by earlier philosophers of science. Their strict systems remain important, however, but as part of an order used rhetorically by scientists, rather than an accurate description of successful practice.

> When philosophers carry on their discussions of science in terms of the strict system they are not describing either the cognitive or the material practices of the scientific community, even in ideal form. They are touching on its moral order (Harré, 1986, p. 88).

So

> . . . the importance of the strict system lies not in epistemology, but in the fact that it represents the most perfect and generally sutained moral order ever created by mankind (Harré, 1986, p. 91).

At the level of the moral order there will certainly be plenty of scope for excluding pseudoscience but this will not allow us to demarcate at the level of cognitive or material practices. Instead there will be something like the quality control described by Ravetz (1971). Also drawing on Polanyi, Ravetz (1971) regarded science as craft work and explored "how objective scientific knowledge can result from the intensely personal and fallible endeavour of creative scientific inquiry" (Ravetz, 1971, p. 75, fn 2). Like a craftsman, a scientist constructs and works with objects, intellectual objects, and is the expert in any branch as to what is good and what is bad scientific practice. But craft skills are not normative like demarcation criteria.

> By the use, or abuse, of craft skills, it is possible for slipshod experimental work to be dressed up in a conventional description, for unsound data to be converted into empty information by dubious mathematical techniques, and for an incoherent argument to masquerade as close reasoning (Ravetz, 1971, p. 277).

Likewise, if the critical attitude which Popper made central is one of the cornerstones of science, it is "on a practical, rather than an epistemological basis" (Ravetz, 1971, p. 277, fn 4).

The breakdown of formal boundaries between science and pseudoscience, was no light matter. The simple formula "science has access to truth, pseudoscience does not" no longer held or was deemed secondary. And in the scientific world at large there was serious unrest. This culminated in the "science wars", an upheaval which formed the background to the most recent revival of "pseudoscience".

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RESTORING THE BOUNDARIES: THE SCIENCE WARS

Science wars and the revival of pseudoscience

The first skirmishes in Britain were triggered by a scientist's attempt to restore science to its privileged position in relation to truth. Lewis Wolpert (1992) wheeled out the old shocker that Eddington had used 60 years earlier:

> ". . . physics teaches us that the greenness of grass, the hardness of of stones and the coldness of snow are not the greenness, hardness and coldness that we know in our own experience, but something very different." (Wolpert, 1992, p. 6)

These compelling experiences are a construction.[13] Only science has direct access to truth and reality.

This attempt to raise the flag for science would probably have had little impact if Wolpert had not also vigorously attacked SSK. The implications for funding were potentially serious, and reactions surfaced angrily at a meeting of the British Association for the Advancement of Science in 1994 (reported in the *Times Higher Educational Supplement* Sept 16 1994, p. 44). Wolpert dismissed SSK results as "either trivial, obvious or wrong", and accused the perpetrators of being motivated by envy. SSK's spokesperson, Harry Collins, refused to debate with him at such a personal level, but in the same year Durham University's Centre for the History of Human Sciences did get some of the protaganists together, and proceedings were published in *History of the Human Sciences* (May 1995).

Gross and Levitt (1994) attacked on a broader front than Wolpert. Their target was "the academic left", infiltrated by a postmodernism with a foothold in the whole range of Arts subjects. They were defending science against a politically motivated rebellion: "Modern science is seen . . . to be both a powerful instrument of the reigning order and an ideological guarantor of its legitimacy" (Gross and Levitt, 1994, p. 12). Like Wolpert, they believed that underlying this was envy, due to science's lofty position in the epistemic hierarchy. Unless checked, the rebellion will ultimately threaten the contemporary university, an institution whose "health has become incalculably important for the future of our descendants and, indeed, of our species" (Gross and Levitt, 1994, p. 7). Humanity itself is in danger when postmodernists try to seize possession of the tree of knowledge, and obliterate the distinctiion between science and pseudoscience.

Their chapter on SSK took on Bruno Latour. The attack was personal, the intellectual equivalent of war. Latour's work "provokes and titillates" (Gross and Levitt, 1994, p. 57); "a rather light-footed style is needed to get away with such stuff" (Gross and Levitt, 1994, p. 58), so he needs all of "his intellectual cunning and his seductive charm" (Gross and Levitt, 1994, p. 59). The "stuff" was Latour's "Third Rule of Method", "which drives more earnest and responsible philosophers of science into paroxysms of disgust" (Gross and Levitt, 1994, p. 58). They refuted the Third Rule with a "homely example", of people cooped up in

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a windowless office settling a dispute about whether it is raining or not by stepping outside. "Insofar as we are disciples of Latour, we can never explain our agreement on this point by the simple fact that it is raining. . . . Baldly put, this seems ridiculous" (Gross and Levitt, 1994, p. 58). This goes to the heart of the matter; science's epistemic authority had been challenged, and a new urgency arose, leading to a return with Wolpert to fundamentalism about truth in order to restore the demarcation between science and pseudoscience.

In the year that Gross and Levitt's book was published, the New York Academy of Sciences sponsored a conference with the same theme (Gross et al., 1996). Not all the contributions were on a war footing, and even the chapter on Bruno Latour was within the bounds of peacetime debate. This was not true of Mario Bunge. If you walk

> . . . from the faculties of science, engineering, medicine or law, towards the faculty of arts . . . you will meet another world, one where falsities and lies are tolerated, nay manufactured and taught, in industrial quantities (Bunge, 1996, p. 108)

Civilization is under threat, not from terrorism but from academic charlatans who peddle anti-science and pseudoscience:

> Spare the rod and spoil the charlatan. Spoil the charlatan and put modern culture at risk. Jeopardize modern culture and undermine modern civilization. Debilitate modern civilization and prepare for a new Dark Age (Bunge, 1996, p. 110).

Bunge declared a ten-point Charter of Intellectual Academic Rights and Duties, and concluded with a stirring call to "all genuine intellectuals" to

> . . . join the Truth Squad and help dismantle the "postmodern" Trojan horse stabled in Academia before it destroys them (Bunge, 1996, p. 111).

In spite of the famous victory of Sokal's Hoax,[14] the hostilities petered out indecisively when the defence failed to match this level of aggression (Ross, 1996). Gieryn (1999) described the science wars as violent boundary work. But it was also tree work, angry pruning, with hurt nostalgia for the days when scientists were universally respected as dedicated servants of the tree of knowledge; and pseudoscientists were laughable pretenders, recognisable by their isolation and their paranoia.

AFTERMATH OF THE SCIENCE WARS

The increasing threat from little pseudoscience.

During the 1990's, in the midst of all this distressing activity, writings on little pseudoscience acquired a new urgency. Later books, including several by Gardner

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himself, continued to stress their educational function. But his hope for a more enlightened public had not been realised: "Science education in our nation, especially in lower grades is getting worse, not better." (Gardner, 2000, p. 3). Pseudoscientists were no longer isolated cranks, but well organised and with enough popular support to sooth their paranoia.

Sagan (1995) was equally worried about the new threat. As an astronomer he paid special attention to UFOs and alien abductions. The boundaries are not secure since "a kind of Gresham's Law prevails in popular culture by which bad science drives out good" (Sagan, 1995, p. 6); and being error prone we need training in skeptical thinking (Sagan, 1995, p. 21). But educational immunization is not enough, the problem goes deeper. Thus the thoroughly educated Professor John Mack, a Harvard University psychiatrist and Pulitzer prize winner, came to believe the testimony of those who claimed to have experienced alien abduction. Mack proposed "the very dangerous doctrine that 'the power or intensity with which something is felt' is a guide to whether it is true" (Sagan, 1995, p. 153). The danger is within, and we must learn to question even our strongest feelings.

Michael Shermer (2002) is the director of the Skeptics Society, and founder of the *Skeptic* magazine, which has been warning the world about pseudoscience for several years. The first edition of his book in 1997 ended with a chapter titled, like the book itself, "Why do people believe weird things?" The second in 2002 added a new chapter, "Why do *smart* people believe weird things?". The confirmation bias that influenced reports of the testing of Einstein's eclipse predictions, was a danger for very smart people indeed—Eddington, Einstein, and Popper! Education is not enough. In a note added to the 2002 edition he brought the urgency up-to-date; the mass suicide by the Heaven's Gate UFO cult in 1997 proved that weird beliefs are not harmless fun (Shermer, 2002, p. xxvi).

Not only has the nature of the fear changed, but also the target. Pseudoscience, big or little, can no longer be held at a distance. Like terrorism, it is felt as a danger to us all, an internal not an external threat, and not open to ordinary debate. It is these fears that have spilled over into journal articles whose mission has been to expel new therapies into the wilderness of "pseudoscience". But there is disagreement about the wisdom of such stern methods. The issue of *Scientific Review of Mental Health Practice* referred to above, was a discussion of a lead article from Richard McNally by a panel that included Mario Bunge and some of the authors of the EMDR article (Herbert et al., 2000). Disagreeing with them, McNally concludes that "the best way to debunk bunk in clinical psychology is to examine the relevant evidence. Attempts to diagnose pseudoscience are an unnecessary and roundabout way of achieving the same goal" (McNally, 2004, final page).

In the same spirit Lilienfeld et al. (2003), contains careful examinations of intractable difficulties in achieving a scientific psychotherapy. On the one hand, the Editors' introductory chapter refers to "the festering problem of pseudoscience *within* clinical psychology" (Lilienfeld et al., 2003, p. 10, our italics) and offers a

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list of ten criteria for the detection of pseudoscience; and the book ends with a five-point recommendation for mobilisation ("the battle against pseudoscience is too substantial to be waged on a single front", Lilienfeld et al., 2003, p. 462). On the other hand, many of the chapters are not about pseudoscience, but address the problems that threaten the scientific security of the best clinical psychology. They bring out the difficulties in scientific regulation of psychotherapy, compared with patent medicines and food.

One problem arises from the distinction between efficacy and effectiveness (Seligman, 1995; Garske and Anderson, 2003). Efficacy is how well the therapy works under controlled conditons; effectiveness is whether it works in practice. But they may not always coincide. And what if pseudoscientific mumbo-jumbo is effective therapy? Is that a case for Bunge's Truth Squad? A second problem is the placebo effect (Walach and Kirsch, 2003). In medical research the ordinary placebo effect is allowed for by double-blind randomly-controlled trials. In trials of antidepressant drugs there is usually a large effect of both the drug and the placebo, with a relatively small difference between them. This relatively small difference could in principle be explained by an active placebo effect, in which the side-effects of an active drug trigger a placebo response. There is evidence that active placebos are better than inert placebos, so that the apparent drug effect in clinical trials could sometimes be largely a placebo effect, with subsequent therapeutic effectiveness pumped up by the publicity surrounding individual anti-depressants. This is disturbing news, since drug trials are usually regarded as the gold standard to which psychotherapy can only aspire. What happens to the aspirant when the gold turns out to be counterfeit? Does psychotherapy focus attention on the special nature of its own problems (as McNally recommends, or instituting what Ravetz, 1971, referred to as "quality control"), or are the anxieties turned outwards in a derogatory labelling of others that includes an unstated self-definition as scientific. The book amply illustrates both answers, as well as the difficulties that trigger the insecurity.

The Disunity of Science

The comforting force of an implicit self-definition as scientific rests on an assumed identity with other scientists, and hence on a unity in science. But is there much in common between the struggle to find a way of controlling for placebo effects in psychotherapy, physicists investigating the reality of Quarks during the 1970s (Galison, 1997, pp. 642–643), biological taxonomists classifying a newly discovered species, or metereologists coordinating data to predict tomorrow's weather? Is there really a tree of knowledge whose unity provides security even for its remotest branches?

Suppes (1978) examined the assumptions behind the Unity of Science movement. The potential unity is based on three possibilities; reduction of language,

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of subject matter, and of method. He argued that none of these is compatible with modern science, and concluded that science is not

> . . . an ever closer approximation to a set of eternal truths that hold always and everywhere . . . (but) . . . is perpetual problem-solving. No area of experience is totally and completely settled by providing a set of basic truths; but rather, we are continually confronted with new situations and new problems, and we bring to these problems and situations a potpourri of scientific methods, techniques, and concepts, which in many cases we have learned to use with great facility (Suppes, 1978, p. 14).

Other writers have followed, philosophically (Dupré, 1983; Rosenberg, 1994), historically (Galison, 1997), and both together in the chapters of Galison and Stump (1995). Kuhn's theory posited discontinuity between different periods of a science, in the form of incommensurability. The synchronic version of this starts from "the extraordinary variety of scientific languages, practices, purposes, and forms of argumentation" (Galison and Stump, 1996, p. 13), not just between the sciences, but within a science such as physics. This might suggest a mapping task, drawing boundaries before looking at the action within each paradigm. But Galison has focussed on the areas between the disunified parts of science, the "trading zones" (Galison and Stump, 1996, p. 13). Rather than worrying about paradigms or frameworks which create incommensurability problems, Galison made incommensurability the norm and argued that "the different subcultures of science. . . . work out local trading zones in which they can coordinate their practices" (Galison and Stump, 1996, p. 14). The study of these trading zones in recent physics has proved a fruitful celebration of disunity (Galison, 1997).

Hacking (1996) has given a clear summary of the issues. Having exposed the disunity behind the deceptively simple facades of a reductionist unity of language and of subject-matter, he turned to that of method and reason. Unity might be an ideal, but in practice there have been many different styles of scientific reasoning. Whereas Galison had found disunity within a limited time and place, Crombie (1994) uncovered six styles in his massive history of Western Science since the Greeks. There is unity as well, based on science's committment to argument and evidence, and its belief in rational causality (Crombie, 1994, p. 4), but no methodological essence to shore up the tree of knowledge, or justify the mission implied in "Unity of Science". In particular, there is unlikely to be a settled unity between the methods of the natural and the human or social sciences. Different methods and styles of reasoning evolve from the objects discovered and investigated, like craft skills which are shaped and perfected in relation to the objects constructed. Merely to import methods from successful natural sciences whose objects are well established, into social and human sciences whose objects remain diverse and unclear, can only produce an illusion of unity. Foucault (1970, pp. 347–351) had acknowledged the local importance of mathematics and formal experimentation in the human sciences, while insisting that these cannot be constitutive of its objects. Drawing on this work, Hacking (2002) argued that in the natural sciences the objects constituted through experimental methods are timeless

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(the object known as the photo-electric effect goes on manifesting itself in applications such as the automatic opening of supermarket doors). But the categories of the human sciences are themselves historical, so that "We remake the world, but we make up people" (Hacking 2002, pp. 49–50).

CONCLUSION: DIFFERENT PSEUDOSCIENCES

We have explored how the technical use of "pseudoscience" is centred around a perceived threat to the security of science, or a sub-culture of science, or an interest threatened by another's scientific pretensions. The reassurance achieved depends on asserting the accuser's own participation in the methods and reason of the unity of science, as a branch of the tree of knowledge. One and the same process prunes the enemy from the tree, grafts the self to it. The threat is likely to be felt most strongly in the branches that are least secure in their status of science, like the least eligible members of a club that confers status on all its members equally. Pseudosciences themselves do not have an obvious unity. If they fall short as science it is in very different ways; there is not much in common between, for instance, psychoanalysis and dowsing , but they acquire a reflected unity by shared exclusion from a unified science.

So what if science is not a unity, if there is no tree of knowledge, and the technical sense of "pseudoscience" has little validity? What if the club is an illusion and the labels "science" and "pseudoscience" are like the badge saying "courage" given by the Wizard of Oz to the cowardly lion (which of course worked, like a placebo!). Each use of the word will then spring from the specific problems and insecurities of the sub-culture in question. We end by viewing pseudoscience from this point of view, and distinguish ten versions, ten aspects of pruning the fantastical tree of knowledge.

1. False pretension to knowledge.

The first pseudoscience is a subtle and disciplined system, often with a passion and an immediate relevance that could tempt the uncommitted student away from even the intellectually most exciting parts of science. It is a *Weltanschauung* that poses as an alternative vehicle of empirical knowledge. Marxism and Psychoanalysis were the examples taken by Popper. The status of science itself is at stake.

2. Degenerating research programme.

Here the focus is less on alternative disciplines, than on a potential pathology within science itself, the demarcation "between scientific and pseudoscientific

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adjustments" (Lakatos, 1970, p. 117). It is not an alien force, or a wilful attempt to gain from boasting scientific credentials, but a disease to which any science is prone in the absence of proper vigilance.

3. Ideology.

This draws attention to exceptionally powerful areas of science that refuse to question flawed presuppositions shored up externally, and threaten to stifle progressive alternatives.

4. Going beyond the soluble.

Failing to be content with areas within reach of current scientific psychology, and speculating outside the boundaries. Gentle guidance is required, especially for students of psychology who yearn for something more dramatic.

5. Individual cranky theories.

This use of "pseudoscience" fortifies the individual reader's sense of his or her rationality and allegiance to science. It is educational, warning the public that the outlawed activities should not be mistaken for science.

6. Organised cranky theories.

This emerges out of 5. The outlawed actitivies have becme well established themselves and are getting out of control, so more drastic action is necessary.

7. Dangers from within.

Pseudoscientific tendencies are in us all; it is essential to remain alert to them, or science itself may be at risk.

8. Unregulated medicine.

The label "pseudoscience" draws attention to failures or absence of regulation of patent medicines, and asserts the need for scientific standards against market lawlessness. Such disorder threatens both the public and established manufacturers.

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9. Unregulated therapy and cranky theory.

This is the use by Herbert et al. (2000), and their pseudoscience is a mixture of 6 and 8. They attack EMDR both for claiming but not demonstrating success as therapy (a regulation issue) and for the crankiness of their theory (a demarcation issue).

10. Suppression of pluralism.

Finally we invent our own pseudoscientific psychology, reflecting our anxieties about the Truth Squad. William James is sometimes seen as the father of modern psychology, but his paternity is paradoxical since he founded no school. That may be because he was self-consciously a pluralist, interested in many psychological topics and tolerant of all (including some now excluded as pseudosciences). He allowed for the diversity and lack of clarity in Psychology's objects, and the discipline, in spite of attempts to unify, has followed James in this respect. Disunity was his legacy. This is a source of scientific strength rather than a weakness, since the trading zones that follow from disunity are not just places for exchange, but also for fertilization,[15] and ultimately, perhaps, for clarifying psychology's objects and evolving the methods (the craft skills) appropriate to those objects. In which case a unified psychology, enforced by the Truth Squad, would deserve to be dismissed as an example of the very thing it abhors, a pseudoscience (in the everyday, rhetorical sense).

SUMMARY

We have looked at the different uses of the word "pseudoscience". We considered its origins in the emblematic tree of knowledge and later the twentieth century demarcation debate, which attempted to lay down boundaries around a unified science. At the same time the word was used in popular writings to distance science from cranky theories with scientific pretensions. We viewed the use of "pseudoscience" as an indication of changing anxieties about science and about being a scientist during the course of the twentieth century. The word has asserted the scientific credentials of the user at the same time as it denies these credentials to the pseudoscientist. But the urgency of this assertion has varied with the varying insecurities of the period and of the user. It dwindled around 1980 when philosophical attempts to find a formal demarcation petered out, and the growth of social constructionism denied science any special access to truth. The reaction to this led to the science wars in the 1990s, when a coalition of science marched to restore its privileges with a fundamentalist view of truth. This ushered in a new urgency in the use of "pseudoscience", especially from the least secure branches.

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We argued that the technical use of "pseudoscience" implies a unity of science, a privileged tree of knowledge or space from which the pseudoscience is excluded, and the user's right to belong is asserted. By questioning this unity, recent writings potentially undermine the validity in its use (other than in the everyday rhetorical sense of an activity falsely claiming scientific status), and suggest that the comfort drawn from it is based on an illusion. From this sceptical viewpoint we concluded by summarising the different uses uncovered in this paper.

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NOTES

[1] "It is in fact pointless to attempt to articulate a principle delimiting the scientific from the non-scientific" (Newton-Smith, 1981, p. 91); ". . . there is no single satisfactory philosophical answer to the question of how to demarcate science from non-science" (Dolby, 1996, p. 162).

[2] Typically the pseudoscience is revealed by the absence of a list of 10 qualities derived from Bunge (1984). Science, unlike pseudoscience, admits its own ignorance, advances knowledge by posing and solving new problems, welcomes new hypotheses and methods, has testable theories and hypotheses (always, pseudoscience only sometimes), looks for examples that contradict its beliefs, etc. (http://teach.lanecc.edu/kime/SI%20links.html)

[3] Hanen et al. (1980); Laudan, R. (1983); Wallis (1979).

[4] Magendie (1843, p. 150) referred to phrenology as "a pseudo-science of the present day". We owe this reference to one of the referees.

[5] Discussed in Gieryn (1999).

[6] Some writers put a hyphen between "pseudo" and "science". We have not pinned down a consistent pattern in this, though little pseudoscience (e.g. Gardner, 1952;

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Young, 1970) seems more likely to have the hyphen than big (e.g. Blum, 1978; Cioffi, 1998). Time may also be a factor, the hyphen being dropped as the word became more familiar.

[7] Blum (1978) called them grand and petit pseudoscience.

[8] This was first published by Putnam as *In the Name of Science* (Gardner, 1952), but sold poorly; after being issued by Dover as *Facts and Fallacies in the Name of Science* it became a best-seller (Gardner, 2000, p. 3).

[9] The Englidh translation (Popper, 1968) first appeared in 1959.

[10] Popper did not dismiss such systems altogether (Popper, 1972, p. 37). Many writers who have taken psychoanalysis as a pseudoscience treat it as worthless (Bunge, 1967; van Rillaer, 1991), but there has also been careful debate which has shed light on both psychoanalysis and demarcation (Cioffi, 1998; Grunbaum, 1984).

[11] This was the principle of reflexivity, which was far from ignored by later sociologists of knowledge, discussed below.

[12] The term has been used by conservative thinkers to berate Marxists and others, but also by Marxists of those who act in the interests of their group or class, but believe they are disinterested (Williams, 1976). So Popper used it of Mannheim, and Mannheim might have cast it back with a different meaning.

[13] Susan Stebbing (1944) is generally credited with having exposed the fallacy in Eddington's argument.

[14] The acceptance and publication by *Social Text* of scientist Alan Sokal's parody of postmodernist use of scientific language (Sokal and Bricmont, 1998).

[15] Good (2000) describes this between the sub-disciplines of social psychology.

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